The chief experimental difficulty would be to originate waves the length of which was only a small fraction of the diameters of the rods.

Since compression at the surface of a solid depends on E, it would be just possible for a precipice to exist where the height and density of the material made the pressure at the base equal to the "crushing" limit. Taking this limit from some of the text-books on the "strengths of materials," it appears that the highest vertical face which could stand is, for granite, marble, or gneiss, 8000–14,000 ft.; various limestones, 5000–9000 ft.; various sandstones, 2500–9000 ft.; and various chalks, 500–2500 ft.

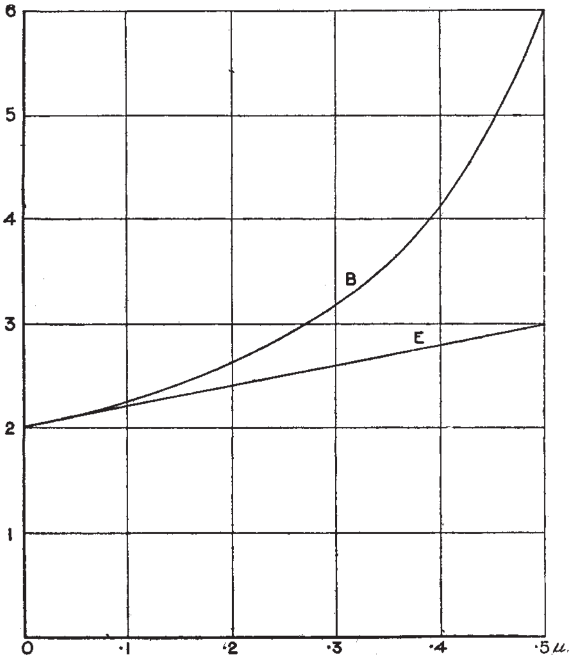

Fig. 1.—The ordinates of the curves show respectively the values of the coefficients E and B as multiples of the rigidity (n) in terms of Poisson's ratio (μ).

I believe that no real precipice (*i.e.* vertical wall of rock) is known which exceeds, even if it reaches, a height of 6000 ft., nor is it to be expected that weathering and other influences would allow of anything like the maximum crushing stress to be borne permanently at a free face.

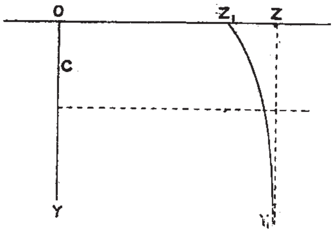

Fig. 2.—OZ is the surface of the ground; OY indicates a vertical plane wave surface; Z_1Y_1 indicates the same surface after advancing in the direction OZ.

If a solid were compressed by a piston in a perfectly inextensible cylinder, the strain produced would not be simple volume compression, but volume compression combined with a certain amount of shear, and if the shearing strain exceeds the shear limit, some molecular rearrangement must take place, although the boundary conditions prevent any actual rupture. The very large differences in the densities of various chalks and limestones may, perhaps, be indications of the pressures to which they have been subjected after their deposition.

A. Mallock.

9 Baring Crescent, Exeter, November 11.

The Action of Sunlight: A Case for Inquiry.

Recently the *Times* published an article by Prof. Benjamin Moore on the action of sunlight, and the ensuing correspondence revealed an extreme contrariety of opinion upon a subject of primary and profound biological and civic importance. Upon such a question men of science should surely have reached some agreement, but it is not so.

Even amongst those who are certain of the superlative powers of sunlight against certain forms of disease, which I have called the diseases of darkness, there is extreme contradiction of opinion as to its *modus operandi,* and this is not merely an academic question, but vitally concerns our urban lives. For about thirty years it has been believed that the healing power of sunlight resides in its ultra-violet rays, which are not light at all in the visible sense. If that be true, it is so much the worse for us in London or Glasgow or Liverpool or Belfast, who are near sea-level, where the solar radiation is very poor indeed in those rays.

I have lately seen the work of Dr. Rollier at Leysin, in the Alps, where ultra-violet rays abound. Since then I have seen for myself the work, second only to his, of the Treloar Hospital, under Sir Henry Gauvain, at Hayling Island, on the English Channel. The view of Prof. Benjamin Moore and Dr. Leonard Hill that the ultra-violet rays are not useful, but dangerous, needing exclusion by the development of pigment in the skin, is thus supported. In a recent letter to a contemporary, *à propos* an article of mine on this subject, Prof. W. M. Bayliss has further questioned the value of the ultra-violet rays, and has pointed to the need for an inquiry.

It is clearly all-important for us to know whether our ordinary English sun, "the light of common day," provides us, at our customary humble levels of business, with enough of the very kinds of radiation which avail for life and against death. I believe that it does, and my belief is strongly confirmed by the reading of the recent papers by Dr. Carl Sonne, of the Finsen Light Institute, at Copenhagen, according to which the healing power of sunlight is due to its familiar, visible rays.

We expect at any moment the final report of Lord Newton's Committee on Smoke Abatement. It will, I am confident, be a cogent and valuable document. Already the time has come, predicted by Dr. Hill a few weeks ago, when we should be longing for the sunlight—and the wise and well-to-do are following the advice of railway advertisements to "Escape Winter Fogs" by going to Monte Carlo. They are, however, "statistically contemptible." The mass of the population, upon whom our Empire chiefly depends in peace and war, have to winter "very otherwise"—here, at least, but not, say, in Winnipeg or Calgary, as I learnt there recently.

Following the publication of Lord Newton's report, let us have a co-ordinated inquiry into the action of sunlight in health and disease, under the fortunate auspices, to which already we owe so much, of the Medical Research Council and Sir Walter Fletcher,

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its secretary. For so wide a theme we shall need the physicist and chemist and biochemist and physiologist and tuberculosis expert and sanitarian, at least.

In the upshot it will appear, I believe, that "the things men live by" (and for lack of which they die), even in our complex and marvellous cities, are very simple. The primary laws remain. As Bacon said, "Nature can be commanded only by obeying her." For the body we need little more than light and air and water and fresh food—*pâté de foie gras* and *eau de Cologne* and champagne are superfluities; and for the soul, as Dr. Cabot, of Boston, says, we need no more (nor less) than work and play and love and worship.

C. W. SALEEBY.

Royal Institution, November 26.

Relativity and Materialism.

MY reply to my friend Mr. Hugh Elliot is quite plainly that, so far as I can discover from his letter in NATURE of December 1, and from his book, and from an article I have read of his, and from an essay which I believe nearly won the five-thousand-dollar prize, he has not understood the principle of relativity. He discusses with very great ability and lucidity the negative results of the experiments and all the illustrations of conflicting experience in relatively moving systems of reference, and, for aught I know, he may be quite familiar with the differential equations which the relativist mathematicians use, but all that he does is to offer a plausible explanation of the phenomena on his materialistic hypothesis. That is not the principle of relativity, yet, strangely enough, he seems quite convinced that it is. The principle of relativity is essentially the construction of the universe from pure concrete experience without any causal theory of experience whatever. This is the very antithesis of materialism. To affirm the contrary is like saying that Berkeley is a materialist. It is simply evidence that words are being used without knowing their meaning.

But let me bring the matter to the test. The universe, according to Minkowski, Einstein, and the other relativists, is a four-dimensional continuum. In this universe there is no simultaneity. This does not mean that we have to calculate the simultaneity of events on a new principle; it means that simultaneity in the accepted meaning has lost all significance, and, in fact, represents nothing; no two events are simultaneous in any absolute or universal meaning. Also in this universe there is no universal system of geometry, nothing which even takes the place of the Euclidean geometry of the Newtonian absolute space. Every point-event has its own geometrical system. The whole rationality of these concepts lies in the conception of a scientific reality constituted wholly of concrete experience. Hence every point-instant in this universe the track of which forms a world-line is taken primarily from its own point of view, according to which it is central and its direction straight.

How can anyone accept this basis of scientific reality and be a materialist? Materialism is a metaphysical theory which may be right, and relativity is an antimetaphysical theory which may be wrong, but acceptance of the one is the rejection of the other.

H. WILDON CARR.

King's College, London, December 2.

PROF. TYNDALL, in his "Scientific Use of the Imagination," allowed a fair play-room to this faculty in scientific research. Dr. Norman Campbell would seemingly restrict its use to a sphere in which phenomena could be submitted to the check of experiment. In his letter in NATURE of November 24, and in his contribution to the Einstein controversy in the issue of February 17 of the present year, he demurs to the use of arguments based on anything that cannot possibly happen.

Dr. Campbell would thus rule out, as scientifically invalid, Prof. Eddington's conception of a perceptive being, travelling at the velocity of light, as having no knowledge of time, and as living in a perpetual present. Such a being and such a condition of things could not be subjected to experiment, and therefore a conclusion drawn from them would be futile.

Einstein's argument based on an imaginary "lift" hanging in space far removed from matter, in which an observer draws deductions, from his experience, as to his gravitational field, would also come under Dr. Campbell's censure, as being outside the region of experiment.

Dr. Campbell seems to me—but I may be quite wrong—to lay down a new canon of scientific method.

EDMUND MCCLURE.

80 Eccleston Square, S.W.1, November 28.

The Radiant Spectrum.

PROF. RAMAN has recently directed attention (NATURE, September 1, p. 12) to some observations by Brewster on what the latter called the "radiant spectrum" (*Phil. Mag.*, vol. 2, p. 202, 1867). Brewster advanced the hypothesis that the phenomenon was due to the granular surfaces of the eye rendering the ultra-violet rays visible by fluorescence. Prof. Raman proposes the alternative hypothesis that diffraction by the corneal corpuscles of the eye accounts for the phenomenon. Brewster's view is at variance with the fact that when a colour screen, opaque to ultra-violet rays, is placed in the optical path between the source, the prism, and the eye, it does not render the "radiant spectrum" appreciably less visible. Fluorescence set up by ultra-violet rays can therefore be safely excluded as a possible cause of the phenomenon. Prof. Raman's view that diffraction effects by the corneal corpuscles of the eye are responsible is at variance with the following facts:—

(1) That if the head of the observer be rotated, so as to rotate the eye about its optical axis, and therefore cause the corneal corpuscles to take up new meridians, then little or no change, such as might be expected, is seen to occur in the radiant spectrum.

(2) That by placing a suitable screen between the prism and the eye, it is possible to exclude the ordinary direct spectrum of the light source and yet still to observe the so-called "radiant spectrum." If the effect was produced by the eye, such a cutting off of the direct spectrum should also have the effect of destroying any diffraction effects produced, since all light would thus be prevented from reaching the eye.

(3) The "radiant spectrum" can be seen on the ground glass of a photographic camera, and can presumably, therefore, be photographed. Since no corneal corpuscles are present in a glass lens the "radiant spectrum" should not, on Raman's hypothesis, be visible in this case.

The following hypothesis fits in with all the above facts, viz. that the "radiant spectrum" originates by diffraction principally at the prism surfaces themselves. Four observations are in favour of this view:—

(1) That a prism with very perfectly polished sides gives a very weak radiant spectrum.

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